We thank the editor and the reviewers for their comments. Referee comments are in black, answers in blue.

1 Editor comments

The presentation and discussion of the results remain very concise and probably fail to render full justice to the nice dataset acquired. The reader would hope for more detailed and in-depth analyses.

Thank you for your comments. The description of the dataset and the results section was extended significantly with several new figures. The discussion was adapted accordingly as well.

- Are there significant snow depth changes after the main snowdrift event? How do the different DSMs acquired after this event compare? Do the computed values of Sx evolve from date to date?

A comparison of the DSMs after the event showing the significant snow depth changes is now shown in a new figure (Fig 9). Sx can be calculated for each DSM, but TLS measurements have only been acquired after snow fall and drifting snow events. Because of this, a time evolution of Sx does not make much sense in this case. In the wind tunnel we could measure Sx during deposition events. There, a time evolution makes sense. This difference is now explained in the discussion.

-Do SMP data reported in Fig.3 correspond to all measurement dates, or only to one date? How does hardness evolve with time after the main drifting snow event? Is there any noticeable trend? How are single-date correlations with Sx? More generally, the potential for detailed diachronic analyses after the main drifting event does not seem to have been exploited.

All measurement dates are shown in this figure (Fig. 10). This is now clearly explained in the caption and the different measurement dates are shown with different colors. How the hardness evolves is now also shown in a new figure (Fig. 6). There is no clear trend. Single date correlations with Sx are no better or worse than the overall correlation. Note that the measurements had to be done on multiple days due to logistical reasons (other field activities, battery recharge for the SMP, etc.)

-Are zastrugis taken into account when computing the wind-exposure parameter Sx? If yes, could these structures perturb the values of Sx, and partially explain the low correlation with hardness? Could it be possible to compute “corrected” Sx values?

Yes, zastrugi are taken into account. The TLS scans are used for the Sx calculation and the zastrugi are in the scans. They could perturb the values of Sx since the zastrugi create measurement shadows, and generally a very rough surface, which makes Sx very sensitive to the settings that are used to calculate it. The correlation is low with any combination of the settings that were used however.

We are not quite sure what is meant with “corrected Sx values”? We don’t think that the presence of zastrugi leads to wrong Sx values per se.

-In section 4, it is said that Fig.5 shows that drifting snow is a necessary, but not sufficient, condition for wind packing. I do not see how this figure proves the “not sufficient” part. Which data do the authors have to support this conclusion?

That’s a good remark. It is true that this is not very clear in that figure (Fig. 7). It can be seen that after the small drifting event on 28 December, many of the SMPs are still very soft. But in that particular case, all remaining SMPs acquired after the main drifting snow event, did have hard snow at the surface. However, Fig. 4 also shows soft SMPs that were acquired after main drifting snow event. This is now explained more clearly in the discussion section.

The results presented in the paper are somewhat disappointing, in the sense that hardness changes appear to correlate relatively poorly with the factors investigated (distance to dune tail, Sx). Comparisons with wind-tunnel experiments are not fully conclusive. Expanding the analyses and discussions along the lines suggested above, might also contribute to increase the impact of the paper.

The analysis and discussion were extended significantly, but it is true that the results are mainly negative, in the sense that we show which parameters are not important. With this dataset it is very difficult to know which parameters would be important to explain the variability of the hardness change. This is addressed and some suggestions are made in the discussion. These problems only concern the small scale variations in hardness measured after the main drifting snow event. The big changes occurring during the main drifting snow event (and the absence of changes before) could be well explained with the amount of drifting snow that had taken place.

In line with the need for more detailed analyses, additional figures showing, for instance, the evolution of the monitored dune with time, or maps of the Sx parameter, would probably be interesting.

Several additional figures were added to the manuscript, including Fig. 9 which shows the evolution of the monitored dune with time.

2 tc-2018-36-SC1

Thank you for your comments. Mainly due to the comment about the evolution of the hardness of the old snow surface, we had another detailed look at the TLS data to try to better distinguish between new snow and old snow surface. We ended up finding some problems with the scans (patterns, misalignments) that were subsequently corrected as best as possible. This resulted in some small changes in the original results and the TLS scans and their accuracy are now presented in more detail.

Page 1 Line 4: Misuse of the word "topology". You are only speaking of the topography, and at a scale of 10 cm. Topology is also not mentioned later in the actual manuscript. (https://en.wikipedia.org/wiki/Topology)

Thank you for pointing this out. We changed "surface topology" to "small-scale topography'

Page 2 Line 1: Clarify how the drifting snow events resembled your wind tunnel experiments earlier in the manuscript. The similarity described in section 4 is lacking. It appears the only similarities were snowfall without wind, and a slow increase in average wind speed.

Some more information was added in the introduction. It's not actually the drifting snow event itself that is similar, but the whole period. Namely the sequence of distinct events with a snowfall, wind without drifting and then wind with drifting is what makes the observed period similar to our experiments. The beginning of the discussion section was adapted accordingly as well. In particular, there we added a comparison of the initial conditions (new snow density and hardness) which were very similar between Antarctica and the wind tunnel.

Page 2 Line 24-25: What sort of measurement error exists for these snow surface hardness measurements? This is of considerable concern as the snow surface is notoriously difficult to measure accurately with an SMP. This error quantification should be included in the subsequent analysis and correlations.

According to our experience and as shown in several previous publications (e.g. Proksch et al. (2015), 10.1002/2014JF003266 or Hagenmuller et al. (2016), 10.3389/feart.2016.00052) the SMP is a very reliable instrument. It's true that there is a surface effect in the top 4.3 mm, when the measuring tip is not yet completely in the snow. This is mainly a problem for calculating density, SSA and other derived parameters from the SMP signal, which should not be done that close to the surface. The force measurement itself, however, should be reliable there as well. It might not be possible to compare it to other types of hardness measurements, since the cross-sectional area of the measuring tip is not constant but this should not be relevant here since we only compare

SMP measurements to other SMP measurements. Lastly, the SMP hardness we use is based on the topmost cm of snow and is not a force value right at the surface.

Page 3 Line 2: Why was the wind direction not measured with one of the 4 anemometers? If you insist on using dune orientation as a meteorological measurement, what is the response time of a dune reorienting? It is stated this was an old dune. How much can we trust this orientation for the current wind events.

There were some problems with the measurements of the wind direction unfortunately. The orientation of the anemometers and CSATs was measured, but some of them have to be wrong, because the resulting wind directions do not correspond to each other. However, the measurements show that the wind direction was very constant (within about 20 and during most of the period within 10) during the main drifting snow event, consistent with the notion of katabatic winds as the main source of the events. We can therefore be quite sure that the direction of the dunes represent this wind direction very well. This is now briefly explained where the main wind direction is introduced.

Page 3 Line 10 (and throughout): At what height were the SPC measurements made? It is inaccurate to say there was no "drifting snow" if the measurements were made above any possible heights of transport. Additionally, please define "drifting snow." Does this mean saltation? If so, the measurement height of the SPC is critical.

The height of the SPC measurements varied between 13 and 24 cm. It's true that what is measured by the SPC does not correspond to the total mass flux, but if the SPC measures no passing particles at all it is safe to assume that there was no significant snow transport happening. The mass flux follows a height profile and does not suddenly drop to zero above a so-called saltation layer. Drifting snow refers to saltation. All this information was added to the manuscript.

Page 3 Line 13: "was about" not "were about"

This section was rewritten.

Page 3 Line 24: It is not obvious in figure two that there are barchan dunes "everywhere." Please highlight the dunes (and distinguish from Zastrugi) or remove this sentence.

This figure (Fig. 8) shows only a single barchan dune (with zastrugi at the tail). A new figure (Fig. 2) was added to the manuscript that shows the TLS scan in a much larger area. In this figure, the barchan dunes are visible in the whole area.

Page 4 Figure 1 (& Page 7 Line 6): There is considerable time lapsing after the "main event" during which there was "no drifting" and subsequent SMP measurements were made. The conclusions in the paper that it was the "main event" that contributed all of the hardening, and not the long period of "no drifting" between SMP's, is not justified unless this temporal span is thoroughly addressed.

We added a new figure (Fig. 6) showing the time evolution of the hardness. It can be seen that the hardness does not increase further after the main drifting snow event. It is therefore likely that the main drifting snow event is the main cause for the hardening. Furthermore, we also did not observe a time effect in the wind tunnel. This is now addressed in the results section and in the discussion section.

Page 4 Figure 1: What is this measurement of wind speed? What length average? What instrument? What height? Which location? Reconsider units for displaying intensity of snow mass transport. Kg m^{-3} s^{-1} would be more insightful.

As explained in the methods section, the wind speed of all sensors was averaged because of long gaps in the data. The caption of Fig. 4 was updated to make it clearer that this averaged wind speed is shown. Instruments, heights and location are all given in the methods section as well. We now also adjusted the wind speeds to a height above ground of 2 m before averaging them. We are not sure what is meant with "length average'? We added the mass flux in addition to the cumulative mass flux to Fig. 4. We think that the cumulative mass flux is important as well because it shows how much drifting there has been in total, before an SMP was acquired for example.

Page 5 Figure 2: It is unclear why this figure was included as it does not add anything to the manuscript that is not already included in Figure 3.

This figure (Fig. 8) was included to show what a barchan dune looks like and to show the zastrugi at the tail of the dune. This is important because the zastrugi indicate that the dune is already partly eroded.

Page 5 Line 2: Fix the sentence that begins with Zastrugi. The citation is improperly included and the sentence is not clear.

Done. Thanks for pointing this out.

Page 5 Line 5: It is not clear from Figure 2A that MOST barchan dunes are shallow. There is one site-specific example.

Yes, Fig. 8 shows an example. However, this dune has quite similar dimensions to values given in the literature (as explained in the text). It is therefore likely that many dunes look as shallow as our example (see also Fig. 2). The corresponding sentence was changed to say "the typical barchan dune" instead of "most barchan dunes".

Page 5 Line 10: This is another fundamental issue in the paper: all the SMPs were disregarded where there was old snow surface was exposed. However, this is very important information as it is a long time asymptotic-like behavior of what will happen with prolonged scouring and "drifting." If there was no change in SMP hardness at these points, show it. If the underlying snow surface hardened even more during prolonged drifting, that is important to know as well. If there was the unlikely softening of the old surface, that is important to know as it puts the other surface hardness measurement in context. If the hardness is uniform surrounding the dune, this could also be used as a very informative normalizing value. The old surface was no doubt evident in the SMP measurements, even when the fresh snow was accumulating, as the SMP gives a profile.

The hardness of old snow is anything but uniform. This can be seen a little in Fig. 4D, where the range of hardness values is huge on most days (except just after the snowfall period). This huge variability masks any temporal effect that may exist. At each SMP position only one measurement is possible and there is basically no way of knowing how hard the snow was in that location at a previous moment in time. Even if an SMP measurement is available close by, this does not guarantee a comparable hardness as the variability is small scale. These are the reasons why we concentrate on new depositions of snow. Due to the large supply of driftable snow after the snowfall period, it can be assumed that most depositions are made of this new snow. And we know how hard this snow was originally and can therefore calculate a hardness change. This is now explained in the manuscript.

The depth of the old snow surface in SMP profiles with newly deposited snow on top is actually not that easy to pinpoint precisely. Usually, it can be done easily with a precision of about 1-2 cm. But this is not precise enough if we then also want to look at the top cm of old snow. The difference to finding the air/snow interface is that the air signal is very flat (in general, a few SMPs had to be removed because this was not the case). The interface is therefore very clear. The signal in the new snow is in some cases quite flat too, but even then varies a lot more than the air signal. Furthermore, the old snow surface can be harder or softer than what is deposited above, at least for SMPs after the main drifting snow event.

Page 6 Figure 3: Again, there needs to be some indication of the temporal evolution of the measurements. They span many days, and this information is and implies that the hardness comparison is at one time step.

The transects are now numbered in the figure (Fig. 10) and the caption says which transects were acquired on which day. The symbols are furthermore colored based on the measurement date.

The same color code was then used in the subsequent figures as well.

Page 6 Line 3-6: See previous comment about discarding data.

The whole explanation about which SMPs are neglected/not analysed and why is now much more detailed, some figures were also added to explain this (e.g. Fig. 5).

Page 6 line 10-Page 7 Line 1: The current "distance to tail" measurement is imprecisely defined. Is this the Euclidean distance or simply the downwind distance? Either way, the crest is parabolic and thus the distance from the tail is not a measurement of proximity to the crest as implied in the previous sentence. A crest is identified in Figure 2. Use this as a line from which to measure up and downwind. It can then be marked where the tail is with a different colored circle in the new figure 4.

As stated in the manuscript, the distance is measured in the main wind direction, it's therefore not an Euclidean distance. The reference for the distance measurement is now stated more clearly and marked in Fig. 10. It's true that the crest is curved and does therefore not correspond to a single value of "distance to tail". But using the crest as a reference is not really an alternative either. The crest is not an accurately defined location. We basically use this term to describe the downwind end of the dune. "Our" barchan dune did not have the almost perfect shape as the example shown in Filhol & Sturm (2015). In such a case, it might be possible to use the crest as a reference.

Page 7 Figure 4: If you are going to use the correlation, show the linear regression on the plot.

Done

Page 7 Line 4: The p-value is small, but there is essentially no correlation. This analysis appears prematurely presented. Surely there is a stronger way to justify the connection. A clustering analysis could be very beneficial for this much scatter.

It's true that the correlation/trend is not very strong. But basically we just present the results as they are. We are not sure what is meant with clustering analysis? We tried using distance bins to reduce the scatter, but this did not help to make the trend clearer or stronger. The discussion section now addresses this in more detail.

Page 7 Line 7-8: Again, the temporal variation in SMP measurements needs to be included in this analysis and on the figures. There is simply too much time elapsing with windspeeds above transport thresholds to lump all these measurements in together. Even more beneficial than what is presented would be the evolution of the hardness!

Done. Figures were added showing the evolution of the hardness (Fig. 6), and in the figures showing SMPs from different measurement days in one plot, the points are now also color-coded.

Page 7 Line 20: Define drifting for the "no drifting" group. Again, SPC height above saltation layers is important to keep in mind here.

As explained in the text, the "No Drifting" group is defined by a cumulative mass flux below 0.25 kg/m^2. Please see also our answer to the "Page 3 Line 10' comment above.

Page 8 Line 10: Again, this correlation is a bit of a stretch, especially with the enormous time elapsed during measurements. If you remove the seven points in the top left corner you would get a positive correlation and negate your results! Explain why these points are so far away from the rest of the cluster.

It's true that the Sx correlation is low and hardly significant. This whole section was modified, as well as the corresponding paragraphs in the discussion.

Page 8 Line 13: Is there evidence the snowfall was homogeneous? A TLS scan to justify this? What about preferential deposition around the old dune?

What we meant actually is that the surface hardness of the new snow was homogeneous. The corresponding sentences were modified and the standard deviations of the hardness are now given in the results section. The new snow height was not very homogenous due to the high roughness of the old snow surface.

Page 9 Figure 6: The significance of conclusions drawn from this scatter plot should be significantly reconsidered. A correlation coefficient of -0.26 is meaningless. And what to think of the measurement error of the SMP for surface hardness?

The measurement error of the SMPs was addressed in a comment above. It is true that the Sx results are not very significant. This section in the results was completely redone and the discussion was adapted accordingly as well. What we did not think of before is that the assumption that the Sx value based on the scan and the SMP hardness change reflect the situation at the same point in time was most likely wrong.

Page 9 Line 1-9: Please expand on the significance of including this paragraph. The wind tunnel comparison appears qualitative at best and at present states that windier conditions result in more wind crusts.

This paragraph was extended a little. But it is true that comparison of the conditions has to remain qualitative with the available data. The comparison of the observed hardness on the other hand is quantitative. We think that comparing the results from Antarctica to those in the wind tunnel is very significant, despite the limitations. In both cases, for example, we observed no wind crust without drifting snow.

Page 9 line 11: The tail area is very wind exposed! Yet it is softer than the crest? This contradicts your conclusions of the importance of wind exposure.

It's true that the tail is generally wind exposed, but what the zastrugi and other surface features do at small scales is difficult to predict. It's possible that the SMPs were acquired in a wind-sheltered area behind a zastrugi. What's more important, however, is that to explain the hardness, the wind-exposure situation must be known at the moment of deposition. As now explained in the discussion, the Sx values calculated based on the scans acquired after the deposition event most likely do not fulfil this condition. I.e. when the snow of the now exposed tail was deposited, it was not wind exposed there, since most of the snow is most likely deposited downwind of the crest.

Page 10 Line 10-12: The conclusions are a bit overstated or inaccurate. This is absolutely not the first time snow redistribution has been quantified in Antarctica, let alone by this institute. This is also not a measurement of "reorganization" as the original location of the snow measured after drifting is not know. The measured changes in associated hardness indicated that during wind transport of snow, there are hardness changes. The "invaluability" of these conclusions is not displayed in the manuscript.

The conclusions paragraph was rewritten to be more neutral. The corresponding sentence in the abstract was also rewritten. It's true that the original location of the snow is not known, but this is probably impossible. We can be quite sure however, that the dunes were formed out of the new snow because so much of it was available for drifting. What's important then is that we know how hard this snow was originally.

Other comments: As particle size is available from your SPC, it would be very beneficial to see the connection between particle distributions changing over time and the increase in snow surface hardness. Undoubtedly, the smaller grains will impact the surface with less momentum, but will also pack into the surface to create crusts. What connection exists between surface hardness and size distributions in your data?

We looked at the particle size distributions during the drifting snow event on 28 December and during the main drifting snow event. During the first event, only particles smaller than 100 microns were detected and the distribution did not change during the event. The wind speed was not that high during this event. This could explain why only very small particles were detected. During the main drifting snow event, a shift in the distribution from larger to smaller particles was detected. This also makes sense, since it can be expected that larger particles are broken up as time progresses. To make a connection to the hardness, we would need temporally resolved hardness measurements while this shift occurred. The SMP measurements at the different stream-wise positions on the dune would more or less provide that (the tail is older than the crest), but there is no way to know which position corresponds to which time. The connection, therefore, cannot be made and on its own, the evolution of the particle size distribution does not really fit in this manuscript.

3 tc-2018-36-RC1

This manuscript introduces the measurements of the snow surface features on the Antarctic ice sheet with the terrestrial laser scanning (TLS) and the SnowMicroPen (SMP) and tries to approach the ind-packing process. First of all I would like to express my sincere respect for authors' effort to obtain the valuable data under harsh conditions. I am quite sure that these measurements and the deduced facts are informative and important to recognize the change in the snow surface features. However, I have got the impression that explanations and the deduced conclusions described here remained qualitative and look wishy-washy. This manuscript needs more polite explanations and robust concluding for the publication. Presumably, at this stage, this manuscript will be more suitable for the "Letters" than the "Article". I suggest followings comments are also taken in to account.

Thank you for your comments. The manuscript was in fact originally intended as a letter and was therefore kept very concise. The revised version now contains much more information and explanations. E.g. the TLS data and its accuracy is described and the explanations about which SMPs are analysed and why or why not are more extensive. There are also several new supporting figures. Looking again at the TLS data, we also noticed some problems that were subsequently corrected and had a small effect on the original results. This is now all explained in the revised version.

Page 2, line 12: Height of the SPC sensor needs to be shown in order to recognize the drifting snow flux introduced below properly.

Done

Page 2, line 19: It looks wind speeds at 1 m and 3.5 m are shown together without adjusting the height. To say the least, it should be attested that the both sensors are out of the boundary layer and averaged wind speed shows the same value when the data logging system worked properly.

We now adjusted the different wind speeds to a height of 2 m above ground before averaging them and this is now explained in the methods section.

Page 2, line 28: Sx is determined with the 1 m search distance in Antarctica. On the other hand, in the wind tunnel experiments, Sx has been obtained with much more short distance based on the measurements with the Microsoft Kinect sensor. I have doubts both can be compared directly since the scales, deduced Sx, are rather different.

That's true, thanks for pointing this out. Sx values from Antarctica and the wind tunnel are now not compared directly anymore in the discussion section.

Page 4, Fig. 1: Air temperature during the observation period needs to be shown as well. Sintering which is strongly depends on the temperature is important for the snowpack hardening. Further, wind direction should be shown not only based on the barchans direction but also the anemometer measurements.

We added a new panel to this figure, showing air temperature and snow surface temperature. There were some problems with the measurements of the wind direction unfortunately. The orientation of the anemometers and CSATs was measured, but some of them have to be wrong, because the resulting wind directions do not correspond to each other. However, the measurements show that the wind direction was very constant (within about 20 and during most of the period within 10) during the main drifting snow event. We can therefore be quite sure that the direction of the dunes represent this wind direction very well. This is now briefly explained where the main wind direction is introduced.

Page 5, Fog. 2: Color code which shows the snow depth change should be shown as Fig.3.
Done

Page 5, line 2: "Zastrugi Filhol and Strum (2015) are" should be expressed as "Zastrugi (Filhol and Strum, 2015) are".
This sentence was corrected.

Page 5, line 4: "in the literature Filhol and Strum (2015)" should be expressed as "in the literature by Filhol and Strum (2015)".
This was corrected.

Page 5, line 5: Do you have any idea why the snow barchans is much smaller and flatter in general than the sand ones?
This is due to sintering. As the snow grows harder, the wind has to become stronger and stronger to keep the dune moving and growing. This limit does not exist with sand. Please see Filhol and Sturm (2015) for a more detailed explanation.

Page 5, line 10: "from before the snowfall"?
The reasons why some SMPs were removed from the analysis are now explained in more detail. The section containing this sentence was rewritten.

Pages 7, and 9, Figs. 4 and 6: Regression line should be also indicated.
We added a regression line to the first figure (Fig 11). The second figure (Fig 12) was modified and there is now no visible trend. That's why no regression line was added there.

Page 8, line 5: Perhaps it is helpful to explain briefly what is the Kruskal-Wallis tests with reference (textbook). Such as, it is a non-parametric method to compare plural independent samples.
A short explanation and reference was added where the test is first used.

Page 8, line 13: "A homogenious snowfall" could be also achieved by the wind tunnel experiments?
Not in the wind tunnel itself. But we collected natural snow and used that in the wind tunnel. The hardness of this snow was always very homogeneous.

Page 9, line 4: Snow mass flux is also within the same range between the observation in Antarctica and the wind tunnel experiments?
As indicated in the discussion, the drifting mass flux was not measured in the wind tunnel experiments.

Page 9, line 6: Since this is the discussion part, authors need to discuss how the higher wind speeds and more intense drifting made the snow surface harder and created the wind packing.
This is now addressed in the discussion section.

Page 9, line 10: Kuznetsov (1960) also observed the hardness of barchans in Antarctica?
Yes. What he measured and why it might be different from our results is now discussed in detail.

Page 9, line 13: I wonder when the trail area had been eroded partly, old snow surface is exposed and hardness becomes larger?

This is of course possible. However, the SMPs we analyse are in locations where the new snow had not been completely removed. Also, in our case, the snow at the tail was actually slightly softer than at other locations on the dune.

Page 9, line 20: As is pointed out before, the scales to evaluate Sx are rather different between the observation in Antarctica and the wind tunnel experiments. Thus, I am not sure the comparison here is reasonable.

Yes we agree, and the different scales are not the only difference. This section in the discussion was rewritten.

Page 10: Although several ideas that brought discrepancies between the direct observations and the wind tunnel measurements are given, all of them seem wishy-washy. Authors say that "the results from Antarctica are less clear". However, I believe that observed one in Antarctica is an undoubted fact and the attempt in the wind tunnel still rooms for further discussions. In fact, no wind pack is confirmed there.

We are not sure if we understand this comment, especially the last two sentences. We observed wind-packing of snow in the wind tunnel many times. Compared to the event in Antarctica, the "hard snow" was of course still relatively soft, but nevertheless a lot harder than new snow. The results from Antarctica are less clear in the sense that we cannot really explain the hardness variability with our data set. The reason the Sx-analysis did not work is most likely due to an insufficient time-resolution of Sx in Antarctica, where only few TLS scans are available to calculate Sx. This had not been considered at the time of submission. The discussion was adapted accordingly.

4 tc-2018-36-RC2

The manuscript presents snow hardness measurements performed in East Antarctica conjointly analyzed with meteorological measurements and snow depth changes to address the wind redistribution process during a drifting snow event and associated snow hardening. This is of valuable interest since only few is known about wind redistribution of snow and associated processes and models would certainly benefit from such observations to develop and evaluate parameterizations in this field. However, even though the manuscript is concise (the format is more that of a brief communication rather than a full-length article) and generally well written, the proposed analysis is incomplete and some of the main conclusions still need to be supported by sounder arguments before publication. In particular, the negative correlation between the wind-exposure parameter and hardness change relies only on a few data points (7 out of 68), making (together with the different environmental conditions, low correlation coefficients and disparity in the numbers used for comparison) the analogy made with the wind tunnel experiments not really convincing.

Thank you for your comments. It's true that the manuscript was very concise and it was in fact intended as a letter originally. The description of the data and the analysis were now extended significantly and several new supporting figures were added. The analysis and discussion of the wind-exposure was revised thoroughly.

1. P1, L16: surface mass balance (not mass balance).
Done

2. P1, L16-17: I understand what you mean but strictly speaking, this sentence seems to describe wind hardening as a deposition process, while its role in terms of surface mass balance is more to prevent further erosion of snow after deposition. Could you reformulate to avoid confusion?

From what we observed during our wind tunnel experiments, wind-packing or wind-hardening is in fact mostly a deposition process. We only observed a significant hardness increase when snow was deposited. This sentence was therefore written like that intentionally.

3. P1, L17: This sounds a bit restrictive, as for instance sintering through thermal metamorphism, the occurrence of melting and refreezing or the occurrence of rainfall can also prevent remobilization of snow.

Metamorphism and melt/refreeze are now also mentioned in that context.

4. P1 L17-19: What is the measurement height of the SPC? How far from the surveyed area is the blowing snow station? This could be of critical importance when interpreting the SPC data (including particle size distribution) since drifting snow is a highly spatially variable process related to highly spatially variable surface snow properties (as shown in Fig. 3).

The measurement heights were added. They varied between 13 and 24 cm. An overview figure was added showing the locations of the meteo stations and the SMP positions in the TLS scan area.

Note: for a matter of uniformity, use either blowing or drifting snow (drifting snow seems more appropriate) to refer to the measurements as well as to the process itself in the whole manuscript, and gives a brief definition of drifting snow (saltation, height of interest etc..).

Drifting snow is now used everywhere. And it is specified, that this term refers to saltation.

5. P2, L25: Is there any mean to quantify the various uncertainties (evoked in the conclusion; see P10, L4) related to the hardness measurements? When possible, you could for instance group the measurements acquired in close locations at the same time to compute a mean value and a standard deviation.

The uncertainties in the TLS scans are now quantified in detail. The uncertainty in the SMP positions cannot really be quantified but is expected to be a few cm at most. We think that the hardness measurement itself is very reliable with the SMP (see e.g. Proksch et al. (2015), 10.1002/2014JF003266 or Hagenmuller et al. (2016), 10.3389/feart.2016.00052). A standard deviation of several measurements would therefore be a measure of the spatial variability of the snow's hardness. Such standard deviations were given for the measurements acquired directly after the snowfall period, to show how homogeneous the hardness of new snow is.

6. P3, L4: Is this direction consistent with the wind directions measured by the Young wind vanes during the main drifting snow event, or with the sastrugi orientation (if measured)?

There were some problems with the measurements of the wind direction unfortunately. The orientation of the anemometers and CSATs was measured, but some of them have to be wrong, because the resulting wind directions do not correspond to each other. However, the measurements show that the wind direction was very constant (within about 20 and during most of the period within 10) during the main drifting snow event. We can therefore be quite sure that the direction of the dunes represent this wind direction very well. This is now briefly explained where the main wind direction is introduced. The orientation of the zastrugi was not measured specifically. However, those visible in Fig. 8 have the same direction as the dune. They were formed during the same main drifting snow event as the dune itself.

7. P3, L10: Change "reaching" for "exceeding".

Done

8. P3, L16: The event seems to involve negligible mass fluxes. You could remove the sentence. As you say later in your description, there is also a small drifting snow event of very low magnitude in early January, but this time qualified as "almost no drifting snow". Please clarify why you consider the first one and neglect the other, or use similar terms to describe them. Again the height of the SPC could help to interpret the magnitude of snowdrift events, as the drifting snow concentration decreases exponentially with height.

The description of the data, including the mass flux data, is now more extensive and all drifting snow events are mentioned and their effects described.

9. P3, L22: or simply this is the hardness of the underlying old snow surface, without being necessarily linked to any deposition event. Irrespective of their "age", drifting and thus unbounded snow grains need to be packed once deposited before exhibiting significant hardness. Hardening also results from changes in the structure of snow with time and temperature. This is something generally not discussed enough in the paper. You should also show and include an analysis of the temperature time serie when discussing the change in hardness over time.

New figures/panels were added showing how the hardness changes over time (Fig. 6) and showing air temperature and snow surface temperature during the investigated period (Fig. 4B). The specific deposition of snow this comment was about is now also addressed in more detail, including a supporting figure (Fig. 5).

10. P3, L23: Surely this is a huge increase when compared to such a small transport amount. The comparison here is not necessary.

We think comparisons of order of magnitudes are often helpful and the remark was therefore left in the text.

11. P3, L24: Do you mean than the barchan dune formed covers most of the sampling area? I only see one barchan dune on Fig. 2. Is the area covered by the TLS large enough to support that other barchans formed "everywhere"?

Yes, Fig. 8 shows an example of one dune. A new figure (new Fig. 2) was added showing most of the scanned area and showing that the dunes formed everywhere.

12. P4, Fig. 1: change "were" to "where" in the caption.

The figure was modified and the caption no longer includes this sentence.

13. P5, L3: When? As there is only one drifting snow event strong enough to cause erosion of snow, it seems that the dune and the sastrugi formed during the same event.

Yes, that is the case. The zastrugi are already visible in the scan from 31 December, directly after the main drifting snow event. This is now clearly stated in the text. The revised manuscript now also contains a figure (Fig. 9) showing the evolution of the dune after the main drifting snow event.

14. P5, L5-6: Are there field reports mentioning numerous barchans dunes scattered over the whole study area? I agree that the bedform on Fig. 2 resembles a barchan dune, but this term refers to a specific morphology that is not clearly encountered on the other deposition areas evidenced by the TLS scans, at least from Fig. 2 alone.

As mentioned above, Fig. 8 shows a single barchan dune, a new Figure (Fig. 2) now shows similar features in the whole study area. We think barchan dunes usually appear in groups. As Filhol and Sturm (2015) explain, they form as snow waves break apart due to a decreasing snow supply. This publication also shows images of barchan fields.

15. P6, L6: As switching back and forth between Figs 1 and 3 is needed to follow your analysis, the use of identical symbols (triangles, circles, squares) that do not contain the same information in both figures can be confusing. Please use different symbols.

Figure 1 (new Fig. 4) was modified and does not show these SMP categories any more. The reason is that the description of the data is now more extensive and the SMP categories previously shown are introduced later in the text. Showing them in this figure would therefore be confusing.

16. P8, L10: Figure 6 mainly shows highly scattered data (a determination coefficient of 7% has no signification). Your negative correlation (which serves however as one the main conclusions

of the study) relies on only 7 points (top left corner) out of 68 points. Why do these 7 points locate out of the cluster? Do they correspond to a particular location on the dune?

The analysis of the Sx data was improved and the whole section about Sx in the results, including the figure, and the corresponding section in the discussion was rewritten.

17. P8, L13: If the atmospheric conditions cannot be compared directly (see P9, L9) and the measurement conditions are quite dissimilar (see P9, L14), thus the observed event is not "a close approximation" of your wind tunnel experiments.

This sentence was changed to "The observed period and the performed measurements were similar to how experiments in the wind tunnel were conducted.' It's true that the main drifting snow event is quite different from what we had in the wind tunnel. It's more the whole period of interest that resembles an experiment in the wind tunnel. This is now also explained more clearly in the introduction. A further similarity are the initial conditions (new snow density and hardness). This comparison was added in the discussion.

18. P9, L11: This is only poorly supported by Fig. 4, and is somewhat confusing since upwind parts of roughness elements are supposed to be more exposed to wind than downwind parts, thus more subject to wind hardening.

It's true that the trend/correlation with the distance is not strong. The discussion section addresses this in more detail now, as well as the second point. It's true that the tail is generally wind exposed, but what the zastrugi and other surface features do at small scales is difficult to predict. It's possible that the SMPs were acquired in a wind-sheltered area behind a zastrugi. What's more important, however, is that to explain the hardness, the wind-exposure situation must be known at the moment of deposition. As now explained in the discussion, the Sx values calculated based on the scans acquired after the deposition event most likely do not fulfil this condition. I.e. when the snow of the now exposed tail was deposited, it was not wind exposed there, since most of the snow is most likely deposited downwind of the crest.

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The paper is interesting but is too short and many details are missing which reduces its potential usefulness for the readers and for future investigations. The tittle and abstract also suggest a wide and comprehensive studies on the topic of wind-packing, but this is in reality only a case study, yet a highly valuable one. Adding the information about the campaign (in the Method section), that may be obvious for the author but are not for the reader, is necessary. This is detailed below in my specific comments. The data analysis (Result section) is well conducted and flawless. Nevertheless, the conclusion about the relationship between hardness and location and worse between hardness and Sx is not convincing. The correlation is significant (under the assumption of normality) but very weak and seems to depend on a few points (that may be outliers, thus breaking the assumption). The result section overstates this relationship. In contrast, the discussion is fair, which leads me to suggest to rewrite the results section in a more neutral / factual way. Otherwise, it is necessary to further explore the statistical robustness using non-parametric indicator, randomization, etc. The publication of the data in a public repository is announced in the paper. This is a good point but is not effective yet.

Thank you for your comments. The data is now described in more detail, including several new supporting figures. The analyses of the correlation between hardness and distance and between hardness and Sx were modified and put into perspective. The corresponding paragraphs in the results and in the discussion were rewritten to be more neutral and factual. The data was now uploaded to the repository and the reference added to the revised manuscript. For the statistical robustness, we use Pearson's and now also Spearman's correlation coefficients and the associated p-values to determine their significance. Kruskal-Wallis test are used to compare different groups of data.

- The title should be more precise and be closer to the actual content of the paper, such as "Investigation of a drifting snow event in Queen Maud Land, Antarctica". Antarctica is wide and diverse, the location is important.

The title was changed to this suggestion.

The abstract needs more details about the location, season and should include some more quantitative information and results such as wind speed, typical annual accumulation, the duration of the observation (e.g. what is "subsequent events" ?). The abstract and title should make clear that the study is not universal.

Information about the location and time of year of the investigated period was added to the abstract. Information about the annual accumulation and other typical meteorological values in this area were added at the beginning of the methods section.

P1 L18: "we" does not include the same authors.

The sentence was changed. This problem was also corrected in other places in the manuscript. Thank you for pointing this out.

P2L1-5: more detail is needed about the location, its climatic characteristics and the time scale of the experiment.

All this information was added at the beginning of the methods section.

P2L15: "the cumulative mass flux.". Starting time is needed.

The starting time was not always the same. In Fig. 4D, the starting date is the beginning of the period of interest, this is clear in this figure since the cumulative mass flux starts at 0. For the results shown in Fig. 7, the starting date was the acquisition time of the last SMP on 22 December. This is now explained in the manuscript.

P2L19: Does it mean that both sensors are at different height , or the height changed over time during the experiment ? Add information about temperature during the experiment which is very important for sintering.

The sensors were installed at different heights above the ground, but the snow depth varied also a little during the experiments. These changes were comparatively small however. The wind speeds are now adjusted for the different measurement height before averaging them. The revised manuscript explains how this was done. Fig. 4 now includes a new panel, showing air temperature and snow surface temperature during the whole period of interest.

P2L20: How many days?

Scans were acquired on nine days during the period of interest. This sentence was changed accordingly.

P2L21: Please add information about the height of measurements, intrinsic precision and actual temperature versus operating temperature specified by the manufacturers. What maximum angle is used and surface area is scanned ?

All this information was added to the manuscript, except for the "maximum angle". We are not quite sure what is meant with that. The scanner was set to acquire points to above the horizon.

P2L23: "About 450 SMP profiles were acquired" along 3 transects in ... indicate the dates / number of days.

The sentence is now "454 SMP profiles were acquired on 11 different days over a period of 24 days.' The exact dates are shown later in Fig. 4. The manuscript now also contains a figure showing a spatial overview of all SMP locations.

P2L29: What about the perturbation of the snow? This is why how frequent the transects have been measured is important.

We do not really understand this comment. If perturbations due to walking around the study area are meant: The site was reached by access routes downwind of the measurement sites as much as possible. Furthermore, care was taken to reuse the same access routes, thereby avoiding more disturbance than strictly necessary. Also, the SMPs were done in a region that was upwind of the container, and out of the fetch areas for both snow drift stations, given the climatological dominant easterly winds during bad weather conditions at PEA.

P2L32: "We cannot calculate a time evolution of Sx". This is not clear why. The description of the DSM data suggest the authors have all the necessary data.

This is now explained more clearly. The point is that we cannot measure a time evolution of Sx during the deposition event. This was possible in the wind tunnel. In Antarctica we only measure Sx after the deposition event.

P3L18: is it possible to show the DSM change map overlayed by SMP measurements (as in fig 3)?

Yes, a figure showing a DSM change map and all SMP positions was added (new Fig. 3).

Fig 1 Panel A: Add transparency on black curve or use a thiner linewidth

Panel C: Add transparency on symbols

What about adding a graph with snow heigth variations estimated from DSMs ?

We made the lines thinner in all panels and added transparency to the symbols and arrows, adding transparency to the wind speed curve did not help.

The snow height variations in the DSMs have a high spatial variability, showing averaged values here would not be helpful. We considered adding the data from the snow depth sensors on the meteo stations, but these data are very noisy and contain some gaps. Furthermore, these two point measurements also cannot represent the high spatial variability of the snow depth changes, except maybe during the snowfall event. The change measured by the stations (and the average change based on the DSMs) during the snowfall period is now mentioned in the text.

Figure 2B: The horizontal scale and vertical color bar are missing

A color bar was added. We are not sure if a scale bar makes sense in a perspective view. We added the length and width of the dune in the caption to give an idea of the scale.

P4L8-9: Are the date of acquisitions random with respect to the distance to tail ? What is the correlation and p-value ?

We are not quite sure what this comment refers to? There was only a figure on page 4. The SMP acquisition on the dune was done in clearly defined transects. The new figure 10 now clearly shows which SMPs were acquired on which day. The distance to the tail was not a criterion for choosing which transects to measure. So in that sense, the date of acquisitions was random. The correlation (and p-value) between hardness change and distance to tail are given in the text and in the caption of the corresponding figure (Fig. 11).

Figure 6: It seems that all the negative trend is driven by 6 points, over 68. To some extent, they seems to be outliers, not from the same distribution, which change the conclusion. Is it possible to identify the location of these points and explain what make them particular ?

The analysis of the Sx data was improved and the whole section about Sx in the results, including the figure, and the corresponding section in the discussion was rewritten.

P10: Do you think that the self-organized nature of the Antarctic case can be a cause of the differing results with the experiment ? Maybe add a comment on that.

We are not sure what is meant with "self-organized nature of the Antarctic case". The discussion of the differing results is now more detailed.

L12 P10: It seems fair to cite Q. Libois et al. 2014 (doi: 10.1002/2014JD022361) as well.

Done